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## GENERAL REVIEWS AND SUMMARIES

## VISUAL SPACE

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In the contributions of Watt (25), Karpinska (13), and of Jastrow (12) one finds a certain tendency to lessen the rôle that has long been assigned by many to the strictly binocular factors, namely of convergence and bi-retinal disparity. Watt announces that he has obtained most vivid plastic effects by presenting to the single eye in rapid alternation the usual disparate stereoscopic views. This he best accomplishes by a double projecting lantern in connection with a rotary disk that cuts off one view as it exposes the other, at a rate of about 0.2 sec. for each single phase. He is also convinced by actual trial that depth-effects truly binocular in quality can be had for the single eye by mirroring in rapid alternation disparate views of actual objects; and like effects, too, when there is any rapid translation of the eye, or of objects with reference to the eye. His theory is, that the binocular mechanism is but a special device for obtaining, under conditions of rest, the results which were previously provided for by rapid translation of the eyes, and which are still so obtained for the single eye when all is suitably arranged; in sum, that there is no peculiar or novel psychic quality in binocular vision. Karpinska (13) using both the stereoscope and the verant with exposures of minimal duration finds other ground for regarding bi-retinal disparity and convergence as of lessened importance. It has of course long been known that with instantaneous illumination of the stereoscopic card, and consequently an exclusion of actual change of convergence, there was still a true binocular depth. Karpinska's observations bring out the *noticeably gradual on-coming* of this depth-

effect even with instantaneous exposure, and the probability that some additional and inner process coöperates with and reinforces the prime effect of the disparity. Furthermore, this inner process can produce the full plastic effect without any bi-retinal disparity; and on the other hand the disparity is no guarantee for the on-coming of the plastic effect. For by irregular use, now of identical views, and now of disparate views, it is found that identical pictures in the stereoscope often give a plastic effect, and disparate views give a 'flat' effect; while with the verant the instantaneous view is normally plastic. Jastrow (12), by experiments in which care was taken to exclude the suggestive effect of *knowing independently* the general limits within which the selected depths would range, found that, when dependent on mere convergence, the judgment was subject to much the same aberrations as are found when it depends on mere accommodation. Points of light from five to twenty-five feet away "may be judged as close as five feet, and as far away as one hundred yards." All in all, then, the estimate of the peculiarly binocular factors would by these studies appear to be lowered relatively to the factors common to both binocular and monocular vision.

Von Kries (14), from the experiments of several workers under his direction, finds that in the excentric visual field, as we have long known with regard to the foveal region, our power to discriminate differences of depth is far more precise than is our power either to perceive the doubling of images or to control the convergence of the eyes in such a way as to bring two images to union. The threshold value for depth-discrimination, as we might expect, falls off very rapidly, the farther out we go from the fovea; the indication from these experiments is, that the decline for depth is more rapid than for visual acuity. Von Kries will not as yet, however, affirm this, but prefers to draw special attention to the great precision, which the experiments make evident, in the combined use of the two eyes even for points at a considerable angle from the center of vision.

While von Kries's results thus indicate a close functional connection between excentric points in the different retinal fields, Berger (3) would confine retinal correspondence to a region within about  $10^{\circ}$  of the fovea. Beyond this limit he fails to find, by means of the stereoscope, either diplopy, or retinal rivalry, or binocular color-contrast, or lustre. In these results, if confirmed, there is nothing markedly at variance with von Kries's report, the region observed by whose students slightly exceeded in but a single instance the limit within which Berger would deny the existence of retinal correspondence. On the

other hand, the same investigator finds (1, 2) that toward the center of vision there is often an irresistible impulsion to bring to corresponding positions and consequently to fusion, images that in the first instance lie on points far from corresponding. Thus with the stereoscope he finds that images disparate, occasionally, as much as  $1.5^{\circ}$  apart vertically, and  $2.5^{\circ}$  horizontally, will, in spite of contrary suggestion and in the face of the usual coördination of the eyes, be brought to fusion by a hesitant turning of one eye's line of regard up or down from the common plane. The author perhaps takes no sufficient pains to make clear that the fusion does not actually take place while the images are still upon points far from correspondence. The fact is, merely, that the impulse to upset the usual coördination in favor of a corresponding retinal position is aroused even from points of considerable disparity.

Von Liebermann's contribution (16) has to do with the comparison of central with excentric depth; the determination of a surface all of whose points appear to lie in a frontal plane passing through the fixation point,—a surface composed of so-called 'abathic' points.<sup>1</sup> He finds that this surface may be unsymmetrical, and does not maintain a constant form as the distance of the fixation point changes, but decreases in curvature with increased distance of fixation. The 'abathic' surface, therefore, is not fixed exactly by the conditions that determine the physiological horopter. In closing the account of matters connected especially with binocular vision, mention may be made of two proposals close upon the mechanical side of stereoscopy. Wiener (26) discusses the possibility of obtaining by selective glasses and white lights of different chromatic composition the effect which is now had by the use of red and green light, respectively, for the images of the two eyes. Estanave (5) describes a device by which, from a single plate, different scenes or different aspects of one scene may be observed from different points of view. In principle it is not unlike what we long have had in street advertisement, where with change of place the passenger reads, from the same surface apparently, now one phrase and now another.

With regard to monocular space, Heinrich (9) reports briefly an investigation analogous to that of von Liebermann, already referred to, in the binocular field. For certain selected depths (35, 45, 60, 80 cm.) the attempt was made to find the shape of a surface all of whose points appeared to be at a distance from the eye equal to that of the fixation point. Lines of astonishingly great curvature, concave

<sup>1</sup> *Isobathic* would seem to the present reviewer a more fitting expression.

with reference to the eye, were found as the locus of such isobathic points, and are graphically presented. The curve, as von Liebermann found in his study, may be unsymmetrical. And if by the thickness of the 'line' we represent the width of the zone of apparent equality, the line increases in thickness with increase of the angle of excentricity. Furthermore, even for a single fixation point, the locus of isobathic points is declared to be in some cases not a single line but a series of more or less similar curves, one behind another. Kuntz, who was the observer in these experiments reported by Heinrich, himself describes (15) a further phenomenon in connection with isobathic points. Two such points, it seems, cease to be isobathic after 20-60 seconds' observation. There then occur fluctuations between apparent equality and apparent inequality, which Kuntz has timed. The periods are variable, in his tables, between 0.78 and 11.22 seconds, as extremes. The author senses some connection between these alternations and the familiar fluctuations of perspective in ambiguous drawings.

Visual direction is the topic of a group of studies. Haberlandt's experiments (8) were concerned with the error in the reproduction of visual changes of direction, according as the changes were noted (a) with eye-movements, (b) with head-movements, and (c) with head-movements and eye-movements combined. The results with his two observers were conflicting: the one, emmetropic, made his smallest average errors with eye-and-head-movements, while pure eye-movements gave largest errors; the errors of the other observer, myopic, bore a relation exactly the reverse. Delay in the subject's indication of the direction, a change to 60 seconds, instead of 2 seconds, of time (*i. e.*, lengthened memory of the offered direction) produced no clear and regular result. Various complications, by which, after the stimulus-direction had been given, the observer indicated it only after certain ocular excursions had intervened, very naturally increase the error of the indications. Hofmann's experiments (11) start from the observation by Aubert, that with rotation of the head to left or right upon a horizontal axis, the apparent visual vertical or horizontal suffers a dislocation; and also from the observation of Hofmann and Bielschowsky, that with upright head a rotation of the background likewise dislocated the apparent vertical. The present experiments give the amounts of such dislocation for various angles of rotation of head and of background. In general the dislocation from these two factors if they are in the same direction is in the same 'sense,' and tends to summate; with opposite directions of rotation



of head and background the tendency is for each to neutralize the effect of the other. Interesting asymmetries appeared; with Hofmann himself, for instance, inclination of the head to the right produces a greater dislocation of the apparent vertical than does a like inclination to the left. Nor need the asymmetry of one observer be the same as that of another. The author believes the dislocation, when lines are used for background, to be independent of Zöllner's illusion.

A controversy with regard to visual direction has arisen upon Witasek's observation that when the two eyes look successively at the same actual point (say, by screening now the one eye and now the other) this point does not appear to lie in the same direction in the successive observations. Hillebrand (10) attributes to heterophoria this apparent shift, and holds that, in spite of appearances to the contrary, the visual direction of the point is the same in the successive observations, because in them all it bears an unchanging relation to the total system. Witasek, on the other hand (27), maintains that these shifts are proof that the directions of monocular space differ from those of binocular space, and that the Law of Identical Directions needs amendment. He scouts the attempt to explain the facts by heterophoria when clinical tests give no evidence of its presence; he even suggests that heterophoria has become a mild obsession with Hillebrand. Von Sterneck's contribution to the discussion (24) is, that the apparent shift is found to change in amount and even in direction with different distances of fixation; he rejects both Witasek's and Hillebrand's explanation, and offers instead what he considers a more psychological theory, that we have here an illusion due to the fact that in monocular vision we are employing sight in an abnormal way, for it is normally binocular. Hillebrand curtly replies to Sterneck that he shows ignorance of dynamic heterophoria. The controversy shows scientific acumen, mingled with verbal disagreement, and all suffused with animal heat.

A somewhat kindred problem is touched by Ovio in two of his papers (18, 19), where he contends that the visual angle should be expressed with reference to the 'principal' point rather than to the nodal point, siding thus with Gullstrand and (to all intents and purposes) Helmholtz, as against Listing and Donders. His chief argument is that by change of accommodation we may shift the nodal point without affecting the apparent size of the object. The visual angle, consequently, which varies directly with the size of the image, has its apex not at the nodal point but at the principal

point of the eye. In a like spirit he holds that the visual projection of points should be described, not as along the nodal line universally, but in a direction occupied by the nodal line usually.

Visual acuity for the excentric portions of the retina has been carefully studied by Poschoga (23). He finds, contrary to what is true of dermal space, that the threshold for spatial twoness is in general not so low when the stimuli are applied successively as when applied simultaneously. By each method of application he investigated sixteen different excentric regions: eight at a distance of  $22\frac{1}{2}^{\circ}$  from the center, eight at a distance of  $45^{\circ}$ , lying on the four chief and the four diagonal meridians of the eye. The advantage of the simultaneous over the successive application of stimuli the author believes to be explained at least in part by the fact that with successive application there was a strong impulse to eye-movements; while with the rival method the judgment depended almost entirely on the purely retinal report. In each of these sixteen different regions, and by each method, Poschoga applied his two stimuli, now the one above the other, now above and to the right of the other, now directly to the right, and so on, boxing the compass through the four principal and the four diagonal directions. The threshold values for all these different combinations when plotted gives an interesting system of 'threshold surfaces.' These surfaces are more nearly circular for simultaneous stimuli, while for successive stimuli they are elongated in the direction of the fovea. For this difference of shape an explanation is offered, in which the well-known tendency of memory to distort the unusual into accord with the usual, is given a prominent place. Poschoga finds in his results abundant ground for rejecting the idea that the spatial threshold in excentric vision is fixed by the purely anatomical conditions of the retina.

Ovio (20) makes detailed calculations and drawings to show the changes which various inclinations of a printed page produce in the visual angle of the letters and consequently in the practical acuity; and he reports the result of actual tests. His more important finds are that the reduction of acuity by such inclinations is not the same for isolated letters and for passages of text; nor is it the same for 'caps' and or 'lower case'; nor is it the same for an inclination of the top of a book away from the reader or toward the reader as for a like inclination right-forward or back. With a book of ordinary size laid flat on the table before the reader, perspective distortion can reduce the virtual acuity as much as one half. Perspective considerations favor large letters, short lines, proper spacing of letters and lines, and small format.

Our knowledge of the perception of movement is furthered by certain sections of Marbe's work (17). He finds that when a row of separated points are made to flash in regular succession the appearance of motion thereby induced passes through several distinct stages as the rate of succession is increased: from a jerky motion spatially interrupted, through a jerky motion spatially continuous, to a uniform and continuous motion, and thence to a final stage where the rate is too high to give any apparent motion. The rates of succession that give these different appearances were determined, both for freely-moving and for fixated eyes, the latter condition requiring in general higher rates. I shall make no attempt to give his application of these facts, and of others connected with flicker and with Talbot's law, to kinetoscopic projection.

The problem connected with the apparent difference of size when sun or moon are near the horizon and when either is in the zenith seems almost as enduring as these heavenly bodies themselves. Filehne (6) opposes Zoth's theory that this difference is due merely to the direction of the line of sight, and would have it that, while the direction of this line is of influence, its influence is quite insufficient to account for the full illusion. Filehne has observed the rising moon with elevated line of sight, and the moon aloft with eyes in the primary position, and all without obtaining the results which Zoth's theory would require. When he observes objects through a suitable black tube, however, he finds a marked reduction in the apparent size of objects viewed with level sight. This effect of the tube is due, he believes, to its exclusion of objects in the lower stretches of the field of view, in comparison with which we have been taught by experience to overestimate the size of objects more near the level of the horizon where the greater distances are found. The rising moon viewed through such a tube is reduced in apparent size, he finds, by more than a half. In a companion paper (7) he addresses himself to Zoth's smoked-glass evidence for his *Blickrichtung* theory. Filehne attributes the persistence of the illusion when sun or moon are viewed through smoked glass, not to the mere difference of direction of the line of sight (for with smoked glass, too, he finds the long-risen moon looking small even with eyes in the primary position, and the just-risen moon looking large when viewed with up-turned eyes) but to the inevitable difference of projection to which experience has trained us. We have grown familiar with horizon-objects at great distances, and accordingly see the horizon-moon as farther and therefore larger. Objects seen in directions well above the horizon are not mentally forced to such extreme distances. The apparent shape of the

heavens is, for Filhene, just the sum of the various limiting projections to which long experience has schooled us.

Questions that lead where psychology passes over into philosophy are kept fresh by certain of the contributors. In the study of hemianopsia, Best (4) is interested in the direction of the line that divides the seeing from the blind portion of the field, especially in those cases where this line cuts through the central area without skirting it. He finds, in a case he has studied with especial care, that the dividing line runs not more than  $8.5'$ , angular measure, from the fixation-point, and in a direction within  $7'$  of the true vertical. Such an 'anatomical vertical,' when allowance is made for the peculiar difficulties and errors of measurement here, is close to the apparent vertical in the normal eye; and taken in connection with certain facts which, he believes, indicate that the blind half is still of influence both upon contrast and upon visual direction, Best holds them all as favoring a nativistic view of visual space.

On the other hand, Nativism in the Kantian sense is combated by Pitkin (22) after describing the look of things at the limits of the visual field and on the borders of the blind spot, where he sees present in the flesh spatiality without spatial form. Moreover for many modern writers the Kantian view, he holds, is paradoxically conjoined with a view of evolution whose very setting and make-up is of space and time. Space is thus regarded as a mere form in our experience and at the same time an essential feature in reality itself. Pitkin argues for realism in which extension is not just a 'sign' or 'mental state' but a true cosmic quality.

A systematic treatment in due order, of the more familiar problems of visual space is given in a volume by Witasek (28), the first of a series to be written by many hands entitled *Die Psychologie in Einzeldarstellungen*. The author has attempted in his work to subordinate physiology to psychology. And yet it is a book to which one will hardly go for those full and precise details of psychological discovery which we have come to expect in a treatise on this topic. It leans, by preference, to discussion and the development of a system in which perhaps the most prominent general feature is the attempt to develop the idea of a space-sensation, connected on the one side (like any other sensation) with its 'stimulus,' namely actual space; and on the other side, with the various mental structures in which the raw 'sensation' of space appears psychically modified and elaborated. A report of the more recent literature should not close without mention of the very full review by Pfeifer (21) of the publications of the year 1908, abstracted, classified, and indexed.



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## AUDITORY SPACE

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Recent contributions on the localization of sound deal mainly with the factors on which the perception of direction and distance depends.

Dunlap (1) made a comparison of monaural and binaural localization. The stimulus was given at forty-eight different positions on the sphere described by the arm of Pillsbury's sound perimeter. The observers indicated the estimated locations either by pointing or by marking the position on a chart. When only one ear was used the other ear was closed with cotton and paste. The results are rather indefinite. There seems to be very little relation between the estimated and the actual location of the sounds. Each observer showed preferences for locating all sounds in certain regions, but these preferences varied from time to time. The principal difference between monaural and binaural localization seemed to be that in the former case the sounds were displaced toward the ear in use.

Intensity as a factor in determining distance was the problem of Miss Gamble's (3) investigation. Two series of experiments were made. In the first series it was shown that the perception of liminal differences in distance roughly conforms to Weber's law. In the second series two stimuli were given which were either of the same intensity but at different distances, or of different intensities but at the same distance. The observers, who did not know how the stimuli were manipulated, stated on the basis of their observations what the differences seemed to be.

A large number of judgments were obtained from numerous observers with the positive result that "intensity is the main criterion in estimating the distance of sounds." 'Nearer' and 'louder,' and 'farther' and 'softer' are practically interchangeable characteristics.

The experiment made by Starch and Crawford (4) relates to the measurement of the distance threshold in different directions. Thirteen directions in the right semicircle, on a level with the ears, were tested to determine whether the least perceptible difference in distance of a uniform stimulus was constant in all directions. The measurements on eight observers consistently indicated that the distance threshold is uniform in all directions tested and that it is approximately one sixth of the absolute distance from the head. The tests

also corroborate Miss Gamble's findings that the perception of distance depends mainly, if not entirely, upon intensity.

The most extensive experiments are those of Ferree and Collins (2). Their object was to demonstrate three arguments for the binaural ratio of intensity. (a) Observers naturally more sensitive in one ear should show a constant tendency of displacement toward that ear. (b) Observers in whom a difference in acuity is produced artificially should manifest the same tendency. (c) A natural tendency toward right or left displacement should be subject to correction by making the proper change in the ratio of acuity.

The experiments were conducted by means of Titchener's sound perimeter. The stimulus was a 20,000 vibration Galton whistle tone. The acuity of the ears was determined by the watch test. Ten observers were employed, but the records of only three are given.

One observer whose ears were equally sensitive showed a slight displacement of 2.1 degrees toward the right side. Another observer in whom the ratio of natural sensitivity of the right to the left ear was as 4 to 1 showed an average displacement of 20.3 degrees toward the stronger ear. The third observer in whom the ratio of sensitivity of the right to the left ear was as 3 to 1 showed a displacement of 10.7 degrees toward the stronger ear. Differences in acuity produced artificially yielded similar results.

Individual differences indicated by Dunlap were also found in these tests, but they seemed to be permanent in the same individuals.

A valuable part of this article is the historical survey of the field since the appearance of Pierce's work in 1901.

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## TACTUAL AND KINAESTHETIC SPACE

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In his first paper (1) Basler investigates the problem,—how great must a movement of a stimulating point along the skin be in order, first, that the movement as such and, second, that its direction be

tactually just perceived? The apparatus consisted of a pencil of hard rubber tapering to a point about 1 mm. in diameter; this was inserted vertically near the fulcrum of a one-armed lever, moving horizontally, the free end of which moved along a scale. The magnification on this scale of the movement of the point was about 50. The part of the skin to be investigated was placed on a support over and in contact with the point which was then given various extents of movement along the skin. On the cushion of the forefinger movements of from .02 to .03 mm. were just perceived, with a pressure of from 3g. to 10g. The sensitivity of the forefinger is as good, concludes the author, as that of the retina at a distance from a moving object of 30 cm., at which distance a movement of .02 mm. is just perceptible.<sup>1</sup> The results were unchanged when, instead of a point, a straight edge, or a disc, 4 mm. in diameter, with a rough surface, was used on the skin. On the forearm, about 4 cm. (proximally) from the hump of the scaphoid bone, the threshold was from .5 to 1 mm. These results are smaller than those of Hall and Donaldson,<sup>2</sup> doubtless because the movement of the point was oscillatory (about 10 back and forth movements for each observation) while Hall and Donaldson made for each only one movement.

The perception of direction was not so accurate, the threshold being about .05 mm. for the finger—a smaller sensitivity than for the eye.

Since the thresholds obtained in these experiments were much smaller than those obtained by v. Frey for successive touch, the explanation doubtless is that the moving point occasions slight displacements of the skin which in turn stimulate the sensory touch endings.

In the second paper (2) Basler studies the minimal speed of a distinguishable movement on a touch area. The finger rested on a fixed cork surface with the finger cushion over a hole 1 cm. in diameter; from below an ebonite point (1 mm. across) just touched the finger. This point was fixed vertically to a horizontal rod movable at any desired speed by a clockwork. The end of the rod was provided with a marker recording on a kymograph. A Jaquet chronograph was used for the time line. After the clockwork had been started (the

<sup>1</sup> BASLER, A. Über das Sehen von Bewegungen. II. Mitt. Die Wahrnehmung kleinster Bewegungen bei Ausschluss aller Vergleichsgegenstände. *Arch. f. d. ges. Physiol.*, 1908, 124, 313-325.

<sup>2</sup> HALL, G. ST. AND DONALDSON, H. Motor sensations on the skin. *Mind*, 1885, 10, 557-563.



kymograph having already been set going) the subject was to state whether he felt a movement. The movement of the point was transverse to the finger axis. After practice the threshold was (for the left forefinger) .05 or .06 mm. At the threshold a movement would often not be felt for several seconds. For the ball of the thumb the threshold was about double, a line of hard rubber, 4 mm. long and moved at right angles to its length, being used, however, instead of a point. For the forefinger the *direction* of movement was always distinguishable at a speed of .15 mm. per second. A subsidiary experiment concerned the rapidity with which voluntary movements for the purpose of tactual estimations (*tastende Bewegungen*) of objects are made. A wire arch was placed over the dorsal side of the forefinger and attached to a movable horizontal rod which recorded on a drum. The volar side of the finger tip could thus be moved back and forth transversely over wires of various diameters wound closely about pieces of cardboard. The subject was simply asked to feel the presented surface and decide whether it was smooth or rough. The average normal speed of such movements, in different subjects, was 30 to 40 mm. per second, the slowest being 17 mm. and the fastest 100 mm.

Treves (3) reports a series of experiments on blind children of different ages testing the accuracy with which they could reproduce positions of the limbs, execute movements of given directions and extents, draw lines and figures (verticals, horizontals, squares, right angles), etc. They were designed chiefly to suggest practical exercises in educating the blind.

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## SPACE ILLUSIONS

BY PROFESSOR HARVEY CARR

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Foucalt (10) contributes a critical digest of the literature on the paradoxical illusion and the two point threshold. He is able to suppress or produce the illusion at will mainly by a manipulation of

the order of the series. Suppression of the illusion is a necessary condition for a valid determination of Weber's threshold. Introspective data are emphasized and the phenomenon is explained in terms of tactile imagery. Hémon (11) presents an interesting experimental study of the experience of feeling parts of the body lost by amputation. Some never experience the illusion, some continually experience the lost member and others feel the phantom member only when the stump is stimulated. In this case the sensation is localized at the extremity of the phantom member, and this latter may be made to disappear by a stimulus applied to the opposite limb. A theory as to the spatial localization of cutaneous sensitivity is broached. In discussing the illusions of amputation and Stratton's experiments upon inverted vision, Pick (17) concludes that the basis of reality judgments in hallucinations is to be found in the lack of conflict between the hallucination and the whole perceptual field. Ponzo's article (18) (printed in Italian) is followed by an excellent digest in German by Kiesow (12). It demonstrates by a variety of tests that a contact on any displaced part of the body tends to be localized normally. The Aristotelian illusion is regarded as a special case of the principle. One may obtain also a single touch with two disparate contacts. Ewald (8) gives an apparatus by which this reversal of the Aristotelian illusion may easily be obtained. The strength of the illusion varied with the method of localization used. Miss Cook (6) presents an excellent study of the illusion of filled and empty tactile space. With two groups of subjects, the same area was stimulated successively and the filled magnitude was underestimated. Simultaneous stimulation of adjacent areas gave an overestimation of filled space. The illusion was practically nil when adjacent areas were stimulated successively. The facts are interpreted in the light of introspective data. The illusion can not be correlated with the functional efficiency of visual imagery; it depends mainly upon the distribution and stress of attention and the mode of apprehension of the magnitudes compared.<sup>1</sup>

In his experiments upon the pendular illusion, Ford (9) accepts the after-image and limen hypotheses, though he gives less importance to the latter factor than did Carr. His tests show the strong influence of a third factor, *viz.*, the direction of attention whose effects are interpreted in terms of the law of prior entry. Dodge (7) replies critically to the work of Ford and Carr. He contends that their experiments do not constitute a critical test of his conception because

<sup>1</sup> See Special Review on p. 256.

(1) after-images were not present in his experiments, (2) the presence of gratings and stationary lights offer a fixed point of reference, and (3) an essential condition of his experiment was the presence of true pursuit movements and Carr can not guarantee that this condition was fulfilled, while attention to the peripheral light will destroy such movements. The fact discovered by Ford that the illusion occurs when the peripheral light is stationary is held by Dodge to prove the validity of his conception of the functional inefficiency of eye movement in the perception of motion. Carr (5) adds some new facts to the autokinetic phenomenon. Two types of illusion were noted, in one of which eye movement was demonstrated. Fatigue due to previous constrained eye position has a pronounced effect with some subjects. Strain of the ocular and associated muscles and its resultant fatigue were effective. By the after-image method, no correlation was detected between the illusion and involuntary bulbular twitchings. Stratton (19) finds that a diasclerotic light may sometimes arouse a localization on both sides of the visual field. The light stimulates both sides of the retina and the normal law of projection obtains.

In the field of the geometrical-optical illusions, Lewis (13) concludes that the illusion of contrast is the limiting case of the Müller-Lyer figure and that both are to be explained in the same terms. The maximal effect obtains with variation of length of arms, but not with angular variation, although the position of the maximal effect as the arms are varied in length is a function of the size of the angle. The results confirm the factual statements of Wundt and Heymans. The facts are explained in terms of the opposing principles of confluxion and contrast. The technique and procedure of the experiment are to be commended. Smith and Milne (14) noted that the presence of the vertical marginal line on common ruled paper is influential in the attempt to bisect one of the ruled lines. The effect varies with individuals and the distance of the vertical line from the edge of the paper. Confluence is the principle of explanation supported. Botti's article (3) is mainly an expository summary of results published elsewhere, a fact which renders any critical evaluation difficult. By a number of figures, he shows that in perceiving the direction or extension of a line, one tends to judge it as being perpendicular to a series of parallel crossing lines. This effect can be negated by divers devices. The principle is applied to the figures of Poggendorf and Zöllner and to the angular illusion. The results seem interesting and important.

Becher (1) and de Boer (2) both criticize the Wundtian principles as the sole explanation of reversible perspective. The complexity of the phenomenon is noted and the influence of apperceptive tendencies is emphasized. Besides general observations and critical discussion Becher offers the results of tachistoscopic tests. De Boer noted the first spatial form apprehended in a short fixation and found that relief occurs more frequently than depth, a fact which is consonant with common experience. Habitual motives due to past experience are regarded as the main determining factor. Thierfelder (20) notes a reversible illusion when regarding a rotating wheel from one side. The form reversal is accompanied by a reversal of the apparent direction of rotation. Burmester (4) calls attention to the fact that the illusion is not new and gives historical references.

Müller (16) discusses the apparent brightening of objects when regarded through a telescope at dusk. The objective intensity is decreased and as a consequence of Weber's law the brightness difference between the object of regard and the darker environment is increased. The object is perceived more distinctly and this distinctness is interpreted as an increase of intensity. Müller (15) is concerned mainly with a criticism of an article by Pozděna who assumed that the moon is perceived as projected on the plane of the sky and as a consequence the apparent size of the moon is directly proportional to the apparent distance of the sky. The assumption is criticized on the basis of theory and fact. Several instances are given showing that the two phenomena are independent variables.

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## RHYTHM AND TIME

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The most important recent contributions to the psychology of rhythm are by Woodrow (5, 6). In his first paper he develops a method of converting 'iambic' rhythm-groups into 'trochaic' and *vice versa*, and 'dactylic' into 'anapestic' and *vice versa*, by increasing the internal interval preceding (or following) the accented stimulus, at the expense of the external interval. The amount of increase in the internal interval just necessary to destroy the original grouping, Woodrow names the 'indifference point,' and considers it to be the measure of the 'amount of rhythm' in the original series.

In auditory series with accent (higher intensity) on each second sound, and 'measures' of 1.5 sec., if the external and internal interval were equal the subjects invariably formed two-groups with the more intense sound leading. With every third element more intense than the other two, with 'measures' of 2 sec., the subjects formed three-groups with the stronger sound leading, when the intervals were all equal. In both cases the 'amount of rhythm,' as measured by the 'indifference point,' increased with the relative intensity of the ac-

cented sound. Similar results were obtained with series of electric shocks. In auditory series in which each second or third sound was of greater duration than the others, the subjects uniformly commenced the groups with the longer sound, if the intervals were all equal. In this case, the author claims, the 'amount of rhythm' decreased as the duration of the long sound increased; the reverse however is true if the reckoning is by duration-units—from beginning of one sound to beginning of the next sound—instead of by empty intervals. In accented auditory series in which the rate was varied between 0.5 and 7.0 sec. to the 'measure', the 'amount of rhythm' was found to increase with the rate. Woodrow thinks that the rhythmic grouping is altogether a matter of temporal relation, and that accent (intensity) determines grouping through the overestimation of the interval preceding the accent.

In his second paper (6) Woodrow takes up the effect of pitch-differences on the auditory rhythm. With 'measures' of 1.5 sec. he found that some of the subjects commenced the groups with the higher pitched note, and some with the lower. Increasing the pitch differences produced neither uniform nor significantly large changes in the 'amount of rhythm.'

The most serious criticism of Woodrow's general method is that it may render the subject especially open to suggestion of grouping in accordance with the *terminus ad quem*. His manner of transforming data however—rearranging the series of judgments to bring into them a consistency the subject did not achieve—is astonishing, to say the least.

Two studies by Wallin (3, 4) are worthy of notice because of the importance of the problems upon which they touch. The first paper gives results of work in which the weak element of each third measure of 'iambic' or 'trochaic' auditory rhythm was displaced (delayed) by the small step procedure. The subject was required to classify the rhythms in five categories, from 'excellent' to 'disrupted'; to make a 'time judgment' on the displacement; and to give introspection on points specified in a list of nine items followed by 'etc.' Only one series was taken on each subject. Two rates of stimulation were used: 0.57 sec. and 1.075 sec. from beginning to beginning of the sounds, which were controlled by a form of the Mayer apparatus rotated by a kymograph. Interpretation of the tabulated results is exceedingly difficult, but the reviewer infers that the amount of displacement necessary to take the rhythm out of the 'excellent' class was from 0 per cent. to 33 per cent., according to

subject. How much displacement was necessary to 'disrupt' the rhythm, cannot be found out from the tables. The results of the 'time judgments' are given only in averages for all subjects together; the averages for the just noticeable displacements range from 4.6 per cent. to 7.3 per cent.

In his second paper Wallin (4) gives the results of an attempt to determine the preferred rate of auditory stimulation, using a metronome permitting 31 rates from approximately 40 to 208 strokes per second, and proceeding by a short method of paired comparison. The final preferences of the twenty subjects (one series) range from 0.305 sec. to 1.37 sec. stroke-period. Although eighteen of the subjects grouped the sounds rhythmically, no serious attempt was made to find the relations between groupings and rates—the main point in investigations of this type. The remainder of the paper describes observations on the rhythmic actions by which patrons of the theater gallery accompany the music. It is unfortunate that the author should have allowed either of these papers to go into print in the shape in which it is.

Arps and Klemm (1) sought to determine the position in the interval between auditory stimulations occurring at a uniform rate, at which the visual difference threshold is lowest. The positions used were 0.06, 0.44, 0.78, 1.50 and 1.81 sec. after the auditory stimulus (hammer stroke), which occurred at the rate of 1.96 sec. per stroke. The visual stimulus was produced by three incandescent electric lamps, which were increased in brightness by the momentary (82 sigma) lowering of the external resistance. All stimuli were controlled by the Wirth pendulum. The data obtained show the lowest threshold just before the stroke. The authors sought also to determine the threshold for displacement of one element in a series of eighteen strokes, each third stroke four times as loud as the others, forming six groups ('dactylic') with 0.65 sec. nominal stroke-period. The data show the lowest threshold for the accented stroke, and the highest for the second weak stroke in the group.

Careful examination of the description of these experiments raises the presumption that the experimenters did not take into account the error resulting from the time required for the hammer to fall after the breaking of the current through the supporting magnet nor of the still larger error due to the delay in the heating up of the lamp filament. The current-increment which occurred 150 sigma before the breaking of the hammer-circuit probably produced a light-increment considerably later; possibly after the hammer-stroke.

The 'dactylic' rhythm probably involved an interval of 0.65 sec. between the two strokes, a longer interval preceding, and a shorter following, since the intensity changes here were obtained by changing the speed of the hammer-fall. This presumption is strengthened by the fact that the subjects 'over-estimated' the interval after the accent and 'underestimated' the interval preceding. Many investigations on (or with) auditory rhythmic series have been spoiled by neglect of the hammer-error; as for example, Salow's work on the feeling-character of rhythmic sound groups (*Psychol. Stud.*, 4, 7-8).

Geigel (2) determined the minimal perceptible time-interval between two sounds of nearly the same character. His apparatus was a small cylinder of wood sliding on a rod projecting from a wooden disc, the whole dropped from various elevations onto a sheet of blotting paper. The spindle struck before the rod, the interval between the two strokes being smaller as the height of fall was greater. Computing the intervals from the height of fall and the length the cylinder slid on the rod, 10.66 sigma gave a *doppel* tone, and 9.64 to 5.93 sigma gave a *gespaltene* tone. At the next step, 5.74 sigma, no *Spaltung* of the sound was perceptible. Extreme allowance for the probable effect of air resistance gives 8.10 sigma and 7.02 sigma for the last two measurements, hence Geigel concludes that an interval less than 7 sigma between the strokes was perceptible. With weaker sounds, still smaller intervals sufficed.

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MEMORY, IMAGINATION, LEARNING, AND THE HIGHER MENTAL PROCESSES (EXPERIMENTAL)<sup>1</sup>

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## I. MEMORY AND IMAGINATION

a. *Summaries and Systematic Treatises.*—Several systematic treatises of the general problems of memory and imagination have appeared during the past year. Peillaube's book (33) is the most pretentious contribution of this group; it is essentially a résumé of the familiar phenomena of this field, with an occasional reference to the traditional theories. Its most distinguishing feature is its discussion of affective memory. In common with many other French psychologists, the author assumes the existence of affective imagery. These images he discusses in detail, describing their nature, their typical forms, and their individual variations. He ascribes a prime significance to this group of images since they constitute the basis for the existence of emotional and temperamental types. Renda (38) insists that the process of forgetting is not a merely accidental character of mental function. It is the result of an active process of dissociation; and it furnishes a means by which consciousness rids itself of excess and redundant material. It is analogous with the principle of natural selection in biology; and it is a *sine qua non* of mental evolution. Piéron's book (36) treats of the problems of memory and imagination in an exceedingly comprehensive fashion; and Piéron makes extensive use of the results of other investigators in these and adjacent fields. He brings the phenomena of memory into relation with the general phenomena of retention in inorganic as well as in organic nature. The author even points out that the Ebbinghaus law of forgetting is analogous to the law of auto-catalysis in chemistry. From these inorganic beginnings, the evolution of memory is traced through the animal series to man. The culmination is reached not in the individual but in society, where, as a result of the symbolizing and schematizing function of language, a social memory is evolved.

<sup>1</sup> This summary was prepared with the assistance of Miss Carolyn Fisher. Several papers which have appeared during the past year are first instalments of more elaborate studies. On account of the limitations of space it is necessary to defer consideration of these until the final instalments appear. A special review of Miss Gamble's monograph will be found on p. 254.

Wreschner's monograph (54) and Lipmann's article (26) are essentially summaries of the literature; and Forel's paper (14) is an appreciative review of the work of Semon.

b. *Discussions of General Questions.*—In Balaban's investigation of the relation between mechanical and logical memory (2), lists of words were learned, and retention was tested, chiefly by means of the *Treffermethode*. In one instance the words were learned in an isolated and purely mechanical fashion; while in another, the learner was instructed to establish some sort of associative bond between the pairs of words. Tests of immediate reproduction showed that logical or associative learning was approximately eight times more effective than mechanical memorization. The introspections revealed the number of associations that had actually been established, —accidentally and involuntarily in mechanical learning, deliberately and voluntarily in the associative procedure. And these data enable the author to state that seventy-five per cent. of the associated words were remembered, while only three per cent. of the non-associated words were retained.

Starting out from the observation that a group of relatively dissimilar and complex data may be remembered better than a group of uniform and simple data, Peterson (35) proceeds to examine the inverse relation which obtains between the factors of simplicity and complexity in memorial material. His procedure consists in learning and recalling nonsense-syllables, words, geometrical figures, and the like, which in successive acts of memorization appear in different sizes, in different colors, in different spatial arrangements, etc. He finds that in those cases where meaning remains unchanged, the introduction of a few variations proves to be advantageous; but when the number of variations is increased to three or four, the advantage disappears or even becomes negative. The phenomenon that progressive increase of complexity ceases, at some point in its course, to increase memorial efficiency is probably due to distraction. The author suggests that the better retention of concrete as compared with verbal material may be a product of a more variable and extensive stimulation in the former case; for while verbal material arouses a richer ideational supplementation, it seems clear that, in most learners, imagery plays a less prominent rôle than sensory stimulation.

c. *Imagery.*—Martin (27) aimed to compare movement images which arise when the moving member is seen, and when it is not seen. Her results show that the crucial hypothesis of Müller and Schumann

—the hypothesis that one's idea of movement is most intensive when one sees the moving member—is not universally valid.

Netschajeff (29) is interested in obtaining data regarding the stock of concrete imagery possessed by children. Lists of words were presented to twelve children of about twelve years of age,—each list containing words which were calculated to arouse two visual images, two auditory images, two abstract ideas, two olfactory images, two gustatory images, etc. (!). The boys' memorial types were revealed by their answers to the questions: "Do you learn best by reading silently, or aloud?" "By visual or oral instruction?" (!). The work of van der Noot (49), and of Weil and Nellen (51) is equally unconvincing.

By an exceedingly ingenious arrangement of her experimental conditions Perky (34) succeeded in showing that the perceptual consciousness is not essentially different from the imaginal consciousness, and that an unsuspecting observer wholly fails to distinguish between the two. A detailed examination and comparison of images of memory and images of imagination reveals the following differences. (Visual) memory involves eye-movements and general kinaesthesia, imagination involves steady fixation and lack of kinaesthesia. The former are scrappy, filmy and give no after-images, while the latter are substantial, complete, and may give after-images. The mood of memory is that of familiarity and recognition, and is intrinsically pleasant; the mood of imagination is that of unfamiliarity and novelty, and is intrinsically unpleasant. Memory implies imitative movements, and the correlated organic sensations; imagination implies kinaesthetic and organic empathy. Memory images arise more slowly, are more changeable and last less long than images of imagination. Memory implies roving attention and a mass of associative material, while imagination involves concentrated and quasi-hypnotic attention, with inhibition of associations.

The papers by Abramowski (1), Betz (3), Dugas (12), Moore (28), Peillaube (33), and Piéron (36) also contain discussions of the nature, function and behavior of imagery.

d. *Association*.—Joussain (20) argues that what is usually called an associated train of ideas is rather a metamorphosis, in which a mental content passes through a continuous series of transformations. Every such transformation involves both conservation and creation,—the persistence of old elements, and the appearance of new additions to the complex. The process of transformation is not fortuitous, but is subject to definite laws,—the survival and condensation of

past experience determining the direction of transition from one idea to another.

Ohms (31) attempts to devise a method for the measurement of *unterwertige* associations. He exposes a forgotten datum, and determines the time which is required for its cognition,—the assumption being that the process of cognition will be slow in proportion as the *unterwertige* association lies far below the limen.

Bovet (6) and Kent and Rosanoff (22) have investigated the problem of preferred associations. The phenomenon that different individuals respond to a given stimulus-word with a variety of reaction-words raises the question: What proportion of reaction-words is common to a group of reagents, normal or abnormal? Bovet's paper is a preliminary report dealing with the technical problem of determining the coefficient of community. The investigation of Kent and Rosanoff is much more thoroughgoing. These authors employed one hundred stimulus-words, and one thousand normal reagents; their results are compiled in a series of tables which show the frequency of recurrence of each reaction-word. Their discussion of individuality *versus* community of association is exceedingly interesting and timely, and their frequency tables will prove to be a valuable compend for readers and investigators of this topic.

In Langfeld's study of association (24), pictures of familiar objects served as stimuli; and the instructions to reagents noffered from those ordinarily given in that the response must not contain the name of the pictured object. Langfeld found that negative instruction are effective in determining associations,—i. e., the *Aufgabe* may be negative as well as positive. Suppression is subject to training, and may be developed by practice.

Rusk (41) reports that there is no discoverable correlation between age and rapidity of association. He believes that he is justified in concluding that imagery is not correlated with efficiency, and that thought without imagery is possible.

Jung's paper (21) is a translation of three lectures which describe and illustrate the general principles of the association method of diagnosis, and the light which this method throws upon certain constellations and upon the mental life of the child.

Nunberg (30) finds that expressive movements of the arm and hand are more intensive in 'complex' associations than in indifferent associations; yet he finds that the amplitude of respiratory movements is distinctly decreased by the presence of a 'complex.'

Franz (15) reports that the association method of diagnosis proved



to be inadequate in the case of one of his patients. A subsequent interview showed that the patient had predetermined his associations, and had thereby defeated the purpose of the test. Franz insists that an interpretation of the results obtained by this method is impossible unless the attitude of the patient be known and appreciated. The association method is not, as is ordinarily supposed, an objective method preëminently; the subjective factor is so important that the method must be supplemented by introspection if reliable results are to be obtained.

Ritterhaus (39) also casts suspicion upon the practical applicability of the association method. Jung, indeed, has pointed out that the method is a delicate instrument which can be of service only in the hands of the experienced practitioner; but Ritterhaus shows that, even in the hands of Jung himself, wholly erroneous results and unwarranted inferences have been obtained. The author gives a detailed report and analysis of his own experimental findings, and shows that any positive interpretation of these data is exceedingly problematic.

e. *Learning and Forgetting*.—Starch (44) recommends practice in the acquisition of motor skill as a laboratory exercise, and discusses the curve of learning. Dearborn (11) describes a number of ingenious experiments which are designed to throw light upon the process of learning. Book (4) shows how a knowledge of the details of the *Lernprozess*, and of the optimal periods of work and rest may be of advantage to the teacher. Eberhardt (13), himself a professional violinist, describes a series of violin exercises, by means of which technical skill may be more readily acquired. The author hints that he has discovered the secret of Paganini. The method consists in imagining the desired movement, and in concentrating the attention upon the *innerliche Vorstellung*.

Swift (45) reinvestigates the plateaus in the curve of learning, in an attempt to throw light upon the conditions which give rise to them. He fails to agree with Book in referring them to lapses of attention and effort. He reaffirms his former view, and points out that a period of mental reconstruction is attested by the fact that skill may advance during periods of cessation from practice.

Ruger (40) attempts to "analyze human methods of meeting relatively novel situations and of reducing their control to acts of skill." Twenty-seven individuals, representing various degrees of mental training, solved mechanical puzzles, and described their procedure. Many instances of human learning were found to be char-

acterized by features which are usually ascribed to animal learning alone. The paper contains a detailed discussion of methods of learning, and of the phenomenon of transfer.

Libby (25) reports that the sentence-method of learning a foreign language possesses certain advantages over the word-method. In an investigation of the act of translating Latin words and sentences into German, von Wartensleben (50) finds that while imagery may be present, yet it is to the *Bewusstseinslagen* that the greatest significance attaches in the process of translating. *Bewusstseinslagen* of familiarity, and of understanding or meaning are most important. Both the *Bewusstseinslagen* and the imagery are subject to wide individual variation; and both may be wholly lacking,—in which latter case the translating is done 'mechanically.' With increase of skill in translating, imagery tends to decrease, and *Bewusstseinslagen* to increase, in frequency.

Knors (23) finds that attempted recitation is advantageous in learning because the effort to recall a datum tends to a greater concentration of attention upon that datum, because recitation shows the learner which data are most in need of attention, because it reduces the fortuitous influences which impede passage from one datum to the next, and because it minimizes the novelty and unfamiliarity of the data.

Abramowski (1) investigated the familiar paradox which consists in our being unable to remember what a forgotten datum was, while still being able to remember what it was not,—or, as the author expresses it, the lacuna in memory resists being filled by a false datum. Pictures and words were shown to an observer, and after he had all but forgotten them, other pictures and words, more or less similar to the originals, were introduced for recognition, in order to test the observer's power of resistance to a graduated series of suggested ideas. It was found that forgetting without resistance is a rare phenomenon, and that resistance may exist in both positive and negative form. Abramowski refers the phenomenon to 'generic feelings.'

Dugas (12) reports a case of *déjà-vu* which concerns a total situation, and which ensues in a prevision of the ultimate issue of the situation, *i. e.*, the observer expects that certain events will now happen as they happened on the previous occasion. The author appends a detailed analysis and interpretation of the phenomenon.

Swift's study (46) is concerned with the reacquisition of a motor habit which had originally been acquired six years before the present

study was undertaken. It was found that much of the former skill had been lost, but the process of relearning was exceedingly rapid,—representing a saving of approximately seventy-five per cent.

f. *Unusual Memories*.—Hennig (17) describes his own remarkable memory for numbers. As reported elsewhere<sup>1</sup> he possesses a 'number form' which proves to be a valuable aid to memory. He now describes the affective tones which attach to dates; and reports that 'sympathetic' dates are remembered much better than 'unsympathetic,' and the latter, in turn, better than 'indifferent' dates. Schmutz (42) has accumulated data bearing upon adults' remembrances of experiences of their childhood. Joteyko's paper (19) is a description of certain mental characteristics of Mlle. Uranie Diamandi, a sister of Pericles Diamandi, and herself a 'rapid calculator' of more than ordinary ability.

## II. INTELLECTUAL PROCESSES

Since his earlier findings in the investigation of the 'indifference point' may have been the product of his experimental method, Hollingworth (18) now chooses another method and attacks the problem anew. The law of 'central tendency' in judgment is again found to be in operation; and the author suggests that it is but a particular instance of a very general phenomenon,—a phenomenon which is exemplified, for instance, in our tendency in every-day experience to conceive of a type as representative of a group.<sup>2</sup>

Tomor (48) attempts to confirm the hypothesis that all mental activity is a product of the reflex movements, and that the processes of thought contain both sensory and motor components.

Dearborn's investigation (10) yields the result that an idea comes clearly to consciousness only when we innervate the muscular movements which would express the idea in words; and that even in becoming aware of the form of an object we are, in great measure, dependent upon language. Brown (7) confirms previous findings that the judgment of difference is influenced by the terms in which the problem of discrimination is formulated,—an influence which is referable to the attitude or bias of the observer.<sup>3</sup>

Book (5) calls attention to the fact that the *Bewusstseinslagen*, *determinierende Tendenzen*, and the like, which come into operation

<sup>1</sup> HENNIG, R. Entstehung und Bedeutung der Synopsien. *Zeitsch. f. Psychol.*, 1896, 10, 183-222.

<sup>2</sup> Cf. p. 220 of the June Number of the BULLETIN.

<sup>3</sup> See Special Review on p. 218.

during more advanced stages of practice, are simply the developed forms of the images which had served as directing agents during the earlier stages of practice. And the author suggests that the dispute between the advocates and the critics of imageless thinking is due to the fact that each camp has confined its attention exclusively to the processes which are found at a single level in the development of mental function.

Okabe (32) finds that the genuine belief-disbelief consciousness is of relatively infrequent occurrence in every-day life; nor is it ordinarily an emotional consciousness. Belief and disbelief are consciousnesses of the same kind. They may be constituted of general or localized kinaesthesia, or of visual imagery; or they may be bound up with and incorporated in a particular consciousness of a verbal or visual sort. In the former case, the contents appear specifically as beliefs; in the latter case they appear as the vehicle of belief which itself finds conscious representation only in the course or mode of occurrence of the contents.

### III. PRACTICE, TRANSFER, CORRELATIONS

Thorndike (47) finds that a seven days' practice in adding results in an average improvement of efficiency amounting approximately to thirty per cent. The author calls attention to the surprising fact that such a slight training of old-established and frequently employed associations should yield so great an improvement of function; and he points out that the relation between amount of improvement and initial ability furnishes an index of the parts played by nature and by nurture in determining human achievement.

From examination papers in geometry, arithmetic and algebra, written by eighty-three boys, Brown (9) calculates various correlations of efficiency. The correlation between algebraical ability and arithmetical ability is high; between geometrical and arithmetical ability, low; and between geometrical and algebraical ability, negative. It is suggested that the close relation between arithmetical efficiency and algebraical efficiency is due mainly to general habits of accuracy, and that memory of preceding propositions is the ability most closely related to other forms of geometrical ability in school-boys,—is, in fact, the central ability. In two wholly unconvincing papers, Winch (52, 53) reports certain indications of the existence of 'transfer.'



## IV. ABSTRACTION AND GENERALIZATION

Moore (28) attempts to trace out the mental contents which are involved in the process of abstraction, or the formation and development of general ideas. His method consists in presenting successive groups of geometrical figures which contain a common element, and having his observers describe their process of becoming aware of the recurrence of the common element. Abstraction is found to contain four stages: (1) The group is broken up, and the common element is selected, (2) perceived, (3) retained, and (4) recognized. Moore reports the existence of 'mental categories' which represent objects. These 'mental categories' are neither imaginal nor affective; they are to be regarded as 'the results of past experience'; and they constitute the essence of abstraction.

Schwiete's problem (43) is briefly this: In what form do general ideas or verbal meanings appear to consciousness? And his procedure consists in asking his observers to ideate the meanings of stimulus-words, and to report their experiences. The author's conclusions seem to be as follows: There is no peculiar type of idea whose function it is to signify or represent a concept. The method of accurately envisaging a general idea varies from individual to individual. The consciousness of familiarity constitutes the first step in the apprehension of a general idea; and the vehicle of the familiarity-character may be a verbal image, a constellation of ideas, a logical context into which the word fits, or an affective experience. When the character of familiarity is intensive it may constitute the sole representation of the word. The envisagement of the word may be mediated directly by an image, by a seriation of the word into a familiar logical context or into a constellation of ideas, or by a process of defining.

Piéron (36) describes several attempts to recall names which he had, for the moment, forgotten. He emphasizes his alternations of hope and despair as he alternately felt that he was on the verge of success, or felt that the search was hopeless. He conjectures that these 'intellectual feelings' may be sensations which result from facilitation of cerebral functioning (feelings of comprehension, of recognition) or from inhibition of cerebral functioning (feelings of unfamiliarity, of obscurity).

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## SPECIAL REVIEWS

## MEMORY

*A Study in Memorizing Various Materials by the Reconstruction Method.* ELEANOR A. MCC. GAMBLE. Wellesley College Studies in Psychology. No. 1. PSYCHOLOGICAL MONOGRAPHS. Vol. X, No. 4, 1909. Whole No. 43. Pp. xii+210.

In this Monograph Miss Gamble reports a heroic set of experiments, covering many years, on memorizing the order of series of smells, colors and nonsense-syllables. The reconstruction method as she used it, was as follows: The subject is given the series, one member at a time, at a fixed rate—for colors and syllables,  $3\frac{1}{2}$  or 4 seconds apart; for smells, 5 to  $5\frac{1}{2}$  seconds, or longer if the subject needed more time to get an impression of the smell. He is then given the members in a chance order and required to assign to each its proper position in the series (by putting the bottle or card in its appropriate spatial relations to all others). One examination only of a member was allowed. A 'correct' reconstruction or ordering was one in which the positions were finally correctly assigned. A 'perfect' reconstruction was one in which this was done and in which also (1) no members had been transposed in the course of the arrangement, (2) no members which came next to each other in the series had ever been separated by a gap and (3) no members not belonging in direct sequence had ever been placed in direct sequence. If the series was not, after one presentation, 'reconstructed' perfectly, all the members were again presented to the subject in the proper order and a second attempt at reconstruction was required as before. This continued until the subject made a 'perfect' reconstruction.

The demand made in this reconstruction is like that made in memorizing a vocabulary or other set of paired associates, the difference being that the member of the pair to be remembered is the position of the given member in the series, so that remembering the associate of any one member helps (a false memory of it hinders) one in placing the others.

Miss Gamble finds that, as compared with learning to give the elements of a series and their order, "a relatively small number of presentations of the series was required even at the outset and apart from the effect of practice, "practice had a continuous and marked effect" throughout, and "the increase of difficulty with increase of



series-length was very small as compared with the increase found by Ebbinghaus."

The author examines by various experiments the following possible explanations of the greater ease of reconstruction, that is arranging properly the members or 'links' when given, than of *producing and* arranging them. (1) Less is to be done, the latter requiring both summoning and arranging, (2) the associative bonds may be toward a spatial localization, (3) each presentation is followed by a test, (4) ample time is given in the tests, and (5) more time was given between the members of one series, and between one series and the next.

She concludes that the common tendency to assume that the first of these is an adequate explanation is unjustifiable, since in fact the subjects do in large measure learn what the members of a series are in the course of learning to arrange them. Her evidence concerning the relative magnitude of these five factors is too intricate for summary, but should be studied by everyone interested in memory from whatever point of view. Many important facts appear in addition to those that directly concern the problems which she set herself. For example, it appears that learning to produce and arrange a nonsense series is facilitated by associating each member with a place or number designating its position in the series in addition to associating it with its precursors and sequents in the series.

It is perhaps unfortunate that Miss Gamble followed the tradition of measuring and comparing the energy expended in memorizing in terms of 'repetitions,' 'recitations,' and 'tests,' rather than in terms of time, supplemented by strain or discomfort. The differences between her findings and those of Ebbinghaus, Müller and Schumann, and others are in large measure due to the fact that a repetition (of hers) is not equal to a repetition (of theirs).

Her own results form a very strong argument against the use of a repetition or a recitation as a unit of measure. The fact that the number of repetitions required for long series, say of from 18 to 81 elements, did not increase as it was found to do by Ebbinghaus and others, is explained as chiefly a result of the slow presentation, and the long intervals between repetitions.

It would be interesting to try learning nonsense-series of different lengths as reversed reconstruction-series, letting the associations be 1 (or the certain spot) *zeb*, 2 (or the neighboring spot) *kag*, 3 (or the next spot) *lef*, etc., etc., and the task be to summon *zeb*, *kag*, *lef*, etc., when 1, 2, 3, etc., are given, and to compare the results, on the one

hand with learning to reconstruct the series (that is to summon 1, 2, 3, when *zeb*, *kag*, *lef*, etc. are given, all other conditions of the experiment of the experiment being identical) and on the other with learning the series by merely rereading them.

Incidentally evidence was secured to show that the degree of ability to image smells bore no considerable relation to ability to remember the series containing them, that the exclusion of secondary associations and other means of adding significance to the bare series of smells, colors and nonsense syllables is practically impossible unless the series are presented very, very rapidly or the experiment is wilfully turned into an experiment in inhibiting all natural movements of the mind; that variations in efficiency in memorizing are related to variations in amount of mental work done without rest chiefly through the variations in interest caused thereby.

Of dependence on verbal rather than concrete imagery the author notes that "The figures at their face value seem to show that the subjects who depend most on concrete imagery learned worst." The complexity of the conditions, especially degree of practice, prevented a precise estimate of the correlation. Space forbids mention of others of the many noteworthy observations.

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### TACTUAL SPACE

*Die taktile Schätzung von ausgefüllten und leeren Strecken.* HELEN DODD COOK. Reprint from the *Arch. f. d. ges. Psychol.*, 1910, 16, 3 u. 4 Hft. Pp. 130.

All tests were made with 'passive' stimulation, i. e., the two extents for each comparison were applied to the passively resting arm of the subject, all movement of the arm itself or of the stimulating object along the skin being excluded. The stimuli were applied longitudinally on the volar surface of the forearm, about in the middle. Subjects were kept, where possible, in ignorance of the results, methods etc. In all, sixteen subjects were used. In each test the subject was to compare the length of a '*Vergleichsstrecke*' (V) with that of a '*Normalstrecke*' (N.).

PART I, the 'successive' form of 'passive' procedure. The stimulating points were hard rubber pegs, each tapering to an area 1 mm. in diameter; for 'filled' (*ausgefüllte* = A) extents the distance between centres of the pegs could be varied from .5 to 3.5 cm.; for unfilled

(*leere* = L) extents only the end-pegs were, of course, utilized. The stimulus was applied by means of a handle held by the experimenter, the two extents to be compared (N ranging from 4 to 14 cm.) being given successively, the second parallel to and about .3 mm. distant from the first. The method of minimal changes was employed. The chief result was that all A-extents from 6 to 14 cm. were, in comparison with L-extents, *underestimated*, and this whether the time order of application of stimuli was NV or VN. Introspection showed that the illusion was much influenced by the subjective distinctness or insistence of the 'filling-points' (*Füllspitze*) and attention to the end-points, the former disturbing the latter so as to conceal or minimize the felt difference between the limiting points. Further, whereas Rieber had found that the A-extents, although underestimated when short, were overestimated when long, Cook found no such reversal of the illusion at all. Rieber had explained the underestimation by a 'fusion' of the points when near together, but Cook shows that the maximum underestimation coincides with the maximum subjective distinctness of the points (2 cm. apart).

PART II. In this second and weightiest part of her experiments Cook used an aesthesiometric balance, one arm carrying the aesthesiometer proper, overbalanced, on the other arm, by weights in the balance pan which, when released, allowed the stimulating points carried by the first arm to descend, until mechanically arrested, with the required pressure on the arm. The points were gauged Chinese pig bristles from .105 to .110 mm. diameter. By special devices these were each vertically adjustable so that the whole row of bristle points could be set to conform to the shape of the volar surface of the forearm, and horizontally adjustable so as to secure any desired distance between bristles. Furthermore, any of the bristle holders could be, either singly or in groups, eliminated from the stimulus row, thus allowing, for successive stimuli, a fairly quick shift (6 to 10 sec.) from an A to an L or vice versa. The arm in this series lay in a plaster form and a row of about equally sensitive points, 5 cm. apart and parallel to the longitudinal axis of the arm, was marked for stimulation.

The 'simultaneous' form of 'passive' procedure. N and V were simultaneously applied, not side by side, but end to end ( $A^L$ ). The method of constant stimuli was employed. The subject was required to say whether V was larger than, equal to, or smaller than N. The N-extents were 4, 6 and 8 cm. in length and the points .5, 1 and 2 cm. apart. *General results:* (1) The AN-extents were over-

estimated, the LN underestimated, i. e. an A-extent was always overestimated; (2) this overestimation of A-extents did not result from visual images (*Gesichtsvorstellungen*) but, since it was just as great in two blind subjects as in strongly visual subjects, it was a tactual phenomenon; (3) if the attention was more strongly focused on one of the extents it was, in comparison with the less strongly focused, overestimated; (4) if the basis of judgment was the immediate impression of extent (*unmittelbarer Streckeneindruck*), the amount of illusion increased with increasing length of N; when other factors entered, the reverse was the case; (5) the influence of the number of 'filling-points' was not immediate nor uniform for different subjects but depended on how the extents were apperceived (*Auffassungsweise*). The constant overestimation of the A-extents in this group differs again from the results of Rieber, who found underestimation for short distances and overestimation for long; it was this particular form of simultaneous stimuli (end to end extents) that Rieber used.

*The 'successive' form of 'passive' procedure.* Practically the same procedure as in the experiments of Part I except that the aesthesiometric balance and the method of constant stimuli were used. *Results:* (1) an A-extent was, compared with an L-extent, underestimated. This result agrees with that in Part I above and both differ from the results of the foregoing 'simultaneous' stimulation. *The direction of the illusion appears, therefore, to be dependent on the manner in which the stimuli are presented;* (2) the illusion was greater when the 'filling-points' were closest together (.5 cm.). This fact does not, however, think Cook, argue in favor of Rieber's theory of 'fusion' (see Part I) and against her own result that the degree of illusion of this type increases with the distinctness of the points, since introspection showed in this case that the points most distinctly felt were those closest together. In short, the distinctness of the points is not uniformly a function merely of their increasing distance apart; on what it does depend is not yet determined.

*Combination of the previous two procedures.* In this group the stimuli extents were also successively applied but were spatially placed end to end with, however, a space of .5 cm. between their adjacent ends, so as to avoid stimulating the same end point twice. The result was that when an L- and an A-extent were compared, there was either no illusion, or one quite minimal. This result, along with the previous findings with successive stimuli, shows that in successive stimulation with A- and L-extents the advantage of A as an impression of extent (*Streckeneindruck*) over L is greatly reduced.



It is a matter of regret that space precludes further discussion of this careful, and, in inferences, conservative piece of experimental analysis. The author reaches the general conclusion that the distribution of the attention between the two extents or within either extent doubtless most influences the judgments; she represents essentially, therefore, an '*Aufmerksamkeitstheorie*.'

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## MENTAL HEALING

*Three Thousand Years of Mental Healing.* GEORGE BARTON CUTTEN.  
New York: Charles Scribner's Sons, 1911. Pp. viii + 318.

President Cutten has given us in this book a most extensive and excellently arranged compilation of the phases and practices of the healing art which we now know as 'mental.' The volume is not for the specialist in these matters, the text being predominantly expository and "dealing more especially with the historical side of the subject."

Stating the common law underlying all forms of mental healing to be: "My body tends to adjust itself so as to be in harmony with my ideas concerning it," and pointing out that there is no 'best form' of mental healing, the author proceeds to show that the diversity of means employed throughout the ages has been such as to meet the wide diversities of custom, belief, and inherited superstition that at any given moment were prevailing.

From the early practices in Egypt and in India, among the Jews and among the Greeks, the reader is carried through successive ages down to the various cults of the present day. In general the association of mental healing with religion is held to have been advantageous, since "the devout attitude of mind is one most favorable for the working of suggestion." The twofold influence of the church, on the one hand in increasing human effort in alleviating illness and on the other in perpetuating the easy superstitions of mankind and thus delaying the development of medical science, is well set forth. The non-religious 'healer,' it is pointed out, has been more ready to recognize the limitations of his power and has thus been less obstructive to advances in the healing art than his confrère who has worked in the name of religion.

Separate chapters—and these perhaps the best of the book—are devoted to 'Relics and Shrines,' 'Talismans,' 'Amulets,' and

'Charms.' The material is presented with much fulness but always with discrimination and clearness of exposition. The reviewer is acquainted with no other book from which one may so readily gain an adequate impression of the controlling sway among all peoples at all times of formularies and ceremonies, of incantations and exorcisms, and of all the practices which reveal the trusting faith in the benign power of inanimate objects, nauseous and revolting doses, and absurd rites. Such an historical survey should certainly be read by all those—and there are still not a few of them—who regard the various healing cults as distinctly modern innovations.

The value of the book lies largely in its compilation and convenient arrangement of scattered material and in its marshalling of curious recipes, half sensible or utterly grotesque, to which the credulous afflicted of the past have had recourse. The absurdities of the present may thus be brought into direct comparison, in the mind of the reader, with the (happily) greater absurdities that have been outlived.

The volume closes with chapters, mostly biographical, entitled 'Mesmer and After,' and 'The Healers of the Nineteenth Century.' There is an excellent index.

A. H. P.

#### PSYCHOLOGY OF ADVERTISING

*Principles of Advertising: A Systematic Syllabus of the Principles of Advertising.* DANIEL STARCH. Madison, Wis.: The University Coöperative Co., 1910. Pp. 67.

The content of the volume is adequately expressed in the subtitle: it is thoroughly systematic and it is merely a syllabus. It is a work on applied psychology. There still survives among psychologists a prejudice against applied psychology. This prejudice would be justified if 'applied' psychology meant merely the attempt to apply established psychological laws or principles to such things as teaching, healing and selling. In so far, however, as applied psychology advances the science of psychology it should be given a place of honor by the professional psychologist. It is the conviction of the reviewer that applied psychology has demonstrated its scientific value. Educational psychology has advanced our knowledge of the processes summarized under the headings of memory and of the learning processes. Psychotherapy has stimulated research on the subjects of suggestion and the relation of the mind to the body. Animal psychology has made distinct contributions to the study of

instinct and the genesis of the higher mental processes. The psychology of religion has given incentive to a study of adolescence and of conversion. Psychiatry has increased our knowledge of the emotions and of personal identity. The psychology of advertising is throwing light upon the subjects of attention and of persuasion.

Starch's book is divided into two parts. The first part (the first 41 pages) is devoted to a study of *attention*. This part has 139 divisions or subdivisions and under most of the subdivisions references to literature on the subject are cited. The following quotation (references omitted) is typical:

- "5. Law of Repetition. Attention value of an object depends upon the number of times it comes before us, *i. e.*, on repetition.
- (a) How frequently should same advertisement be repeated?
  - (b) Is it better to use large space occasionally or small space frequently?
  - (c) How much of an advertisement should be repeated, or be in common to all advertisements of a given article? Should advertisements of a given article be placed in the same position in a given medium?"

Part II. is entitled 'Securing Persuasion and Action.' This part is subdivided into 160 sections or headings of which the following are typical examples (subheads and references omitted):

- "4. Relative persuasive power of the different types of argument.
- 5. Motives appealing to women.
- 6. Appealing to instincts.
- 7. Securing action through imitation.
- 8. Securing action through suggestion."

The syllabus is intended as an outline for a lecture course in which collateral reading, personal observations and experiments are required. Although such a course as here outlined might be particularly adapted to the needs of students in a school of journalism, the course is found to stimulate interest in psychology among regular college students more readily than a similar course in pure psychology.

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